

Remarks on the Early History of the Researches of Astronomers relative to the Spheroidal Figure of the Earth. By the Editor.

The establishment of the spheroidal figure of the earth must ever be regarded as one of the noblest results of modern scientific inquiry. It must be acknowledged that a good deal of misconception has hitherto prevailed respecting the original researches connected with this important problem of mechanical astronomy. It has been generally admitted by writers on astronomy that Newton and Huyghens first demonstrated by strict mathematical reasoning that, under certain conditions as regards the mutual action of its constituent particles, the figure of the earth would be spheroidal, the equatoreal being to the polar axis in a certain assignable ratio, which each of these geometers computed according to his peculiar view of the physical conditions of the problem. In this inquiry, however, the priority in respect to time has been, in some instances, awarded to the labours of Huyghens, upon the evidence of a passage contained in his “Dissertation on the Cause of Gravity.” Delambre, who was at first led to give currency to this view of the subject, had, subsequently, the merit of proving it to be inconsistent with the records of history. From an examination of the early registers of the Royal Academy of Paris and a careful interpretation of Huyghens’ own words, as expressed in the work above alluded to, he demonstrated beyond doubt that the original ideas of the latter on the spheroidal figure of the earth were of a considerably later date than that usually assigned to them, and that, in his solution of the problem for determining the ratio of the equatoreal and polar axes of the earth, the Dutch geometer had been preceded by Newton, whose method he had, in fact, merely applied to his own theory of gravity. It may be remarked, however, that both these points admit of being satisfactorily established by a reference to Huyghens’ work alone, independently of any other source of evidence.

But, in connexion with this subject, there still remains the interesting point to be examined, whether Newton was originally led to conjecture the spheroidal figure of the earth by reflecting on the consequences of the theory of gravitation, or whether he was indebted to some extraneous source for the suggestion of so novel an idea. Humboldt, in a recently published work (*Kosmos*, Band I. s. 172), seems disposed to think that Newton received the first hint on this subject from Cassini’s observations of the spheroidal figure of *Jupiter*. This might seem at first sight to be impossible, since it is generally understood that Cassini first announced to the world his discovery of the spheroidal figure of *Jupiter*, in a paper communicated to the Academy of Sciences in the year 1691, that is to say, four years subsequent to the publication of the *Principia*. Humboldt, however, remarks that Cassini’s earliest observations of the spheroidal figure of *Jupiter* ascend to an epoch which is at least twenty-one years anterior to the publication of Newton’s immortal work. In support of this

statement he refers to a passage in the third volume of Lalande's *Astronomy*, wherein the author asserts that he saw in Maraldi's possession several printed sheets of a fragmentary treatise by Cassini on the spots of the planets, from which it appeared that, at least as early as the year 1666, the Italian astronomer had ascertained the spheroidal figure of *Jupiter*. The illustrious philosopher just referred to has suggested that Newton may have become acquainted with these observations of Cassini by personal intercourse with the scientific men of his day, and thus have been led by analogy to his remarkable conjecture relative to the figure of the earth.*

In the hope of obtaining materials which might throw some light upon this interesting subject, I recently undertook an examination of various historical records, the results of which I have embodied in the following notes. It affords me great pleasure to have this opportunity of expressing my sincere thanks to the President and Council of the Royal Society, who very kindly accorded to me the privilege of examining the early MSS. of the Society while engaged in this inquiry.

Statement of Facts connected with the Early History of the Researches of Astronomers on the Spheroidal Figure of the Planets.

1664, Dec. 14. At a meeting of the Royal Society, Hooke remarked that as everything in nature is liable to change, so the force of gravity might probably vary at the earth's surface. He added that, as it seemed to have an affinity to the magnetic virtue, it might, as in the case of that force, *be more intense at the poles than at the equator*.—(*Journal Book of the Royal Society*, vol. i.)

1666. According to Lalande, the spheroidal figure of *Jupiter* was remarked by Cassini previous to this year.—(*Astronomie*, 3^{ième} edition, tom. iii. p. 335.)

1667, April 3. It is recorded in the Registers of the Royal Academy of Paris, corresponding to this date, that *the experiment with the pendulum is conformable to the theory of Copernicus*.—(Delambre, *Hist. Ast. Mod.*, tome i., Addit. p. lxiii.)

1671. The *Mésure de la Terre* of Picard was published in this year. Alluding to the standards of measure of different lengths which are derivable from observations of the rate of oscillation of the pendulum, the author thus remarks:—"Ces mesures uni-

* M. Humboldt evidently attaches considerable importance to this point in the history of the inquiry respecting the figure of the earth, for he alludes to it in the same work on two subsequent occasions. In support of his views, he cites a passage of Brewster's *Life of Newton*, wherein the author states, that Cassini's observations of the oblate figure of *Jupiter* had *probably* suggested to Newton the idea of the earth having a similar figure. I am gratified to state, that the distinguished philosopher to whose authority M. Humboldt refers has expressed himself perfectly satisfied with the conclusions at which I have arrived in this communication. I am inclined to think that if the evidence here presented had been accessible to M. Humboldt, he would also have been induced to modify his opinions on the subject.

verselles supposent que la différence des lieux ne cause aucune variation sensible aux pendules. Il est vrai qu'on a fait à Londres à Lyon et à Boulogne en Italie quelques expériences, d'où il semble que l'on pourrait conclure que les pendules doivent être plus courts à mesure que l'on avance vers l'équateur: *conformément à la conjecture qui avait été déjà proposée dans l'assemblée, que, supposé le mouvement de la terre, les poids devraient descendre avec moins de force sous l'équateur que sous les poles.*"—(*Mésure de la Terre*, p. 5.)

1673, April 13. In the *Histoire Céleste* of Lemonnier, consisting of a collection of extracts from the Registers of the Royal Observatory of Paris, which was first published in the year 1741, there is to be found the following observation by Picard, corresponding to this date:—"Je commençai à m'apercevoir que le disque de *Jupiter* était *un peu ovale*, et que le plus grande diamètre est toujours suivant les bandes."—(*Hist. Cél.* p. 28.)

1678, Feb. 28. At a meeting of the Royal Society, a discussion arose respecting a remark made by M. Gallet, in an account of the transit of *Mercury*, observed by him at Avignon, on the 7th of November, 1677. M. Gallet stated that when the planet was projected on the sun's disc, it appeared to be of an oval figure, the longest diameter being parallel to the equinoctial.* Mr. Henshaw suggested that the appearance might arise from the refraction of an atmosphere about the planet. Mr. Hooke conceived that the body itself of *Mercury* might be of such a figure; and that it might proceed from the velocity of its whirling round upon its axis, he supposing that the axis of its vertiginous or diurnal motion lies north and south, or at right angles with the seeming motion of its parallel to the ;† that a very swift vertiginous motion on that axis made the body of *Mercury* somewhat of the shape of a turnip, or of a solid made by an ellipsis turned round upon its shorter diameter.—(*Journal Book of the Royal Society*, Vol. vi. p. 55.)

March 7. At the meeting held this day, the remark of M. Gallet respecting the oval figure of *Mercury* was further discussed. Hooke's hypothesis was objected to on the ground that, although such an oval figure would be caused by the whirling round of a fluid body, yet it was probable that the body of *Mercury* is solid; and, consequently, the whirling could have no effect upon its figure. To which Mr. Hooke replied, that although it might now be a solid body, yet that at the beginning it might have been fluid enough to receive that shape; and that, though this supposition should not be granted, it would be probable enough that it would readily run into that shape, and make the same appearance; and that it is not

* This is manifestly the spurious phenomenon (termed the *gutta nigra*) which has been observed in modern times on the occasion of transits of the inferior planets. See Baily's paper "On the Annular Eclipse of the Sun of 1836."—*Mem. Ast. Soc.* vol. x.

† This *hiatus* appears in the original MS.: the reader will have no difficulty in supplying the omission.

improbable but that the water here about the earth might do it in some measure by the influence of the diurnal motion, which, compounded with that of the moon, he conceived to be the cause of the tides.*—(*Journal Book*, vol. vi. p. 60.)

1679. Richer's work, containing the account of his observations with the pendulum at Cayenne, was published in this year. It is stated by the author, that the pendulum was found to oscillate more slowly at Cayenne than at Paris.

1686, July 14. In a letter to Halley respecting the claims of Hooke to the discoveries announced in the *Principia*, Newton thus writes:—"There was another thing in Mr. Hooke's letters which he will think I had from him. He told me that my proposed experiment about the descent of falling bodies was not the only way to prove the motion of the earth; and so added the experiment of your pendulum clock at St. Helena as an argument of gravity being lessened at the equator by the diurnal motion. The experiment was new to me, but not the notion; for in that very paper, which I told you was writ sometime above fifteen years ago, and, to the best of my memory, was writ eighteen or nineteen years ago, I calculated the force of ascent at the equator, arising from the earth's diurnal motion, in order to know what would be the diminution of gravity thereby. But yet, to do this business right, is a thing of far greater difficulty than I was aware of.—(*Guard Book of the Royal Society*, N. 1.)

1686, Sept. 3. Newton thus writes to Flamsteed:—"There is another thing said to be observed of *Jupiter* by Cassini long since; namely, that his diameter from pole to pole is shorter than from east to west. If this were certain, it would conduce much to the stating of the reason of the precession of the equinoxes."—(*Biog. Brit.* art. Newton, p. 3229.)

1687, Feb. 2-9. At the meetings of the Royal Society held on these days, Hooke contended that the figure of the earth is that of a spheroid, the shortest axis of which passes through the poles; and he cites, in support of this opinion, the oval figures of the Sun, *Mercury*, and *Jupiter*, as observed by Scheiner, Gallet, and Cassini, respectively.—(*Journal Book*, vol. viii. pp. 127-129.)

1687, April 6. Book iii. of the *Principia* was presented to the Royal Society. In Prop. xviii. there appears the following statement:—"Sic Jovis diameter (consentientibus observationibus Cassinii et Flamstedii) brevior deprehenditur inter polos quam ab oriente in occidentem."

1690. Huyghens' treatise, entitled, *Traité sur la Cause de la Pesanteur*, was first published.† The author states in this work

* These remarkable *aperçus* of Hooke's on the figure of the earth are also to be found in Birch's *History of the Royal Society*, but they seem to have wholly escaped the notice of scientific writers. It may be mentioned, that the words of the original MS. are closely adhered to in the passages here cited.

† This work was originally published in French at Leyden, in the year 1690, while Huyghens was still alive. A Latin edition was afterwards published at Amsterdam in 1728, along with several posthumous works of the author.

that he was originally led to conclude that the figure of the earth is spheroidal by reflecting on the variable rate of the oscillations of the pendulum in different latitudes, as indicated by the observations of Richer at Cayenne. He also mentions, that shortly after he arrived at this conclusion, he was led to investigate the actual ellipticity of the earth upon his own hypothesis of gravity by the perusal of Newton's researches on the same subject, contained in the *Principia*.

Remarks on the foregoing Statement of Facts.

It will be seen that there exists no necessity for appealing to the authority of Lalande in regard to the question of Cassini having observed the spheroidal figure of *Jupiter* previous to the publication of the *Principia*, since Newton expressly alludes to the observations of the Italian astronomer, both in his letter to Flamsteed, above cited, and also in the original edition of the *Principia*. It is manifest, however, that Cassini did not place much reliance upon his early observations of the ellipticity of *Jupiter*, since we find him asserting, as late as the year 1690, that the planet appeared quite round (*Anc. Mem. Acad. des Sciences*, tom. ii. p. 108).

It is worthy of remark that, although Flamsteed is cited by Newton, in the first edition of the *Principia*, as having ascertained by observation that the figure of *Jupiter* is spheroidal, this positive statement is suppressed in the second and third editions. Instead of the words, "consentientibus observationibus Cassinii et Flamstedii," which appear in the original edition (Book iii. Prop. xviii.), the words, "consentientibus astronomorum observationibus," are used in the second and third editions. It might seem, at first sight, as if this alteration had no special reference to Flamsteed, since the name of Cassini is also omitted. We find, however, in the following proposition of both editions, an express mention of the observations of Cassini, but not of those of Flamsteed; whereas, in the original edition, the same proposition contains no allusion to the observations of either of these astronomers. I shall not presume to suggest what may have been the motive which induced Newton thus to suppress the name of Flamsteed in connexion with so interesting a fact as the independent observation of the spheroidal figure of *Jupiter*. I may state, however, that I have carefully examined the *Historia Cælestis*, and also Baily's *Life of Flamsteed*, but I have not found in them any suspicion of the planet being otherwise than perfectly round. It appears to me that, if Flamsteed had reposed any confidence in his observations, I should have found them recorded in one of the works just cited.

It appears that as early as the year 1667 or 1668, Newton was led to consider the diminution of gravity at the equator arising from the diurnal motion of the earth. As he was then endeavouring to explain the mechanics of the solar system by the principle of gravitation, it can hardly be doubted that the spheroidal figure of

the earth, which is a direct consequence of this diminution, had already presented itself to his mind. It would seem from his letter to Flamsteed, that he had only obtained some vague information respecting Cassini's observation of the spheroidal figure of *Jupiter*, and that he regarded such an observation as affording a valuable confirmation of his own abstract researches on the spheroidal figure of the earth upon which the phenomenon of the precession of the equinoxes (alluded to by him) essentially depends. It is certain, however, from the words used by him in his letter to Halley, "But yet to do this business right is a thing of far greater difficulty than I was aware of,"—that he had not at this time devised the method for computing the ratio of the equatoreal and polar axes which he has given in the *Principia*. The difficulty to which he refers manifestly arose from the complication of the effects of the centrifugal force at the equator with those due to the variation in the intensity of gravity occasioned by the greater distance from the centre of the earth of the particles constituting the redundant matter at the equator.

That Cassini's early observations of the spheroidal figure of *Jupiter* were regarded by Newton as valuable, inasmuch as they served, not to suggest, but to confirm, his own theoretical researches, will further appear evident from the following considerations:—

In 1678, when the question of the spheroidal figure of the earth was discussed at several successive meetings of the *Royal Society*, no allusion was made by Hooke, or any other Fellow of the Society, to Cassini's discovery of the spheroidal figure of *Jupiter*. As this fact would have served to throw much valuable light upon the subject of discussion, we may fairly conclude that it was altogether unknown to any of the individuals who attended these meetings. But if Hooke and his scientific friends in London had not yet obtained any account of Cassini's observations, it is in the highest degree improbable that Newton—who was living in comparative retirement at Cambridge, and whose intercourse with men of science was at this time very limited—should have been better informed on the subject.

It seems probable that the presentation of the first two books of the *Principia* to the *Royal Society* in the spring of 1686, and Halley's active dissemination of a knowledge of the important labours of Newton on the System of the World, may have given rise to much discussion both in England and on the Continent, and that out of this discussion, Cassini's discovery of the spheroidal figure of *Jupiter* (or rather his suspicion of that fact, for it was nothing more) first became known in this country. This view of the subject will appear still more credible when it is borne in mind, that at one of the meetings of the *Royal Society*, held in the spring of 1686, we now find Hooke, for the first time, alluding to the observations of Cassini.

That an important scientific fact closely connected with Newton's researches, might have remained unknown to that geometer

many years subsequent to its discovery, is strikingly illustrated by the circumstance that, at the time of the publication of the second edition of the *Principia*, in 1713, he appears to have been unacquainted with Cassini's comparatively recent communication to the Academy of Sciences, in 1691, respecting the spheroidal figure of *Jupiter*; for, if he had been aware of that announcement, he would assuredly have referred to it in the new edition; whereas, we find merely a vague allusion to the early observations of Cassini. It is in the third edition, which was published in 1726, that Newton for the first time alludes to Cassini's communication to the Academy of Sciences in 1691, the vaguer statement which appeared in the second edition being now altogether suppressed.

It can hardly be doubted, when we take into consideration these various circumstances, that, when Newton first became acquainted with Cassini's early observations of the spheroidal figure of *Jupiter*, he regarded them as valuable merely on account of their affording a confirmation of his own theoretical researches on the spheroidal figure of the earth and the precession of the equinoxes.

About the same time that Newton was led to suspect the variation of gravity at the surface of the earth, or at any rate prior to the year 1671, a suspicion of the same fact was announced at one of the meetings of the Academy of Sciences of Paris. It is worthy of remark, however, that, although Delambre carefully examined the early registers of the Academy, he was unable to discover in them the slightest allusion to the spheroidal figure of the earth.

The earliest distinct announcement of the probability of the earth being somewhat flattened at the poles, as a consequence of its diurnal rotation, is, undoubtedly, due to Hooke. The hints on the subject thrown out by that distinguished philosopher at several successive meetings of the Royal Society in the spring of 1678, afford abundant proofs of the sagacity of their author; but, unfortunately, as in the case of his other speculations on the principle of gravity, he failed to establish his conclusions by a process of rigorous investigation.

It is difficult to ascertain the precise time at which Huyghens was led to suspect that the figure of the earth must be spheroidal. It would appear from the preface to his essay, *On the Cause of Gravity*, that the part of the work which treats of this subject was written after his departure from Paris, and, therefore, subsequently to 1681; and as we further gather from another statement made by him in the body of the same work, that he had been led to suspect the spheroidal figure of the earth *previous* to his perusal of the *Principia*, we may infer that his original speculations on the subject were suggested to his mind on some occasion during the interval comprised between the years 1681 and 1687. His subsequent computation of the terrestrial ellipticity, according to his own ideas of gravity, was suggested by the method given by Newton in the *Principia*.

It may be remarked that the spheroidal figure of the earth was originally suggested to Hooke by the announcement of an ana-

logous fact (which, however, turned out to be unfounded), as characterising one of the other bodies of the planetary system. Huyghens, on the other hand, was led to a similar conclusion, by reflecting upon the variation of gravity at the earth's surface, as indicated by the observations with the pendulum. It is highly probable that Newton's views on the same subject were suggested by purely mechanical considerations.

The following conclusions may be considered as fairly deducible from the facts above stated :—

1. The earliest authentic announcement of the spheroidal figure of *Jupiter* is contained in an observation of the planet by Picard in the year 1673. There are good reasons for supposing that about the same time, or even previously, Cassini was led by his observations of the planet to suspect the same fact; but the Italian astronomer does not seem to have entertained a firm conviction of its existence till about the year 1691. It would appear from the evidence of Newton, that Flamsteed was also one of the early observers of the spheroidal figure of *Jupiter*: this statement, however, is not borne out by an actual examination of the recorded observations of the English astronomer.

2. In the year 1667, or 1668, Newton seems to have been led by his speculations on gravity to consider the effects of the centrifugal force in diminishing the weight of bodies at the equator. It is highly probable that the spheroidal figure of the earth had occurred to his mind on this occasion; but we have no reason to suppose that he was acquainted, at so early a period of his researches, with the method subsequently given by him in the *Principia* for determining the ratio of the equatoreal and polar axes.

3. About the time when Newton first turned his attention to the effects of the centrifugal force at the equator, occasioned by the diurnal rotation of the earth, the probability of a diminution of gravity from the poles to the equator, arising from the same cause, was suggested at one of the meetings of the Royal Academy of Sciences of Paris. It does not appear, however, that any inference was deduced from this fact relative to the spheroidal figure of the earth.

4. In the year 1678, Hooke suggested that the figure of the earth might be spheroidal in consequence of its diurnal motion, but neither on that nor on any future occasion did he assign a method for determining the ratio of the equatoreal and polar axes.

5. Some time during the period comprised between the years 1681 and 1687, Huyghens arrived at the conclusion that the figure of the earth must be spheroidal, but his subsequent determination of the ellipticity, according to his own hypothesis of gravity, was founded on the method given by Newton in the *Principia*.

6. There exist no grounds for supposing that Cassini's observations of the spheroidal figure of *Jupiter* were known in England previous to the year 1686.